H-Diplo Roundtables

Bruce Kuklick. Blind Oracles: Intellectuals and War from Kennan to Kissinger. Princeton University Press, 2006.

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Commentary by Robert Jervis, Columbia University

Blind Oracles is not an ordinary book. Part intellectual biographies, part standard history of important Cold War episodes, part a disquisition on the state of our knowledge and various forms of knowing, and part an attempt to trace the influence of ideas. None of the sections are without interest, but they do not fit together entirely well and the fact that so many are covered means that depth must be sacrificed, although Kuklick does make good use of the letters his voluble subjects exchanged. If there is a central theme here, it is that the intellectuals and advisors had less impact on policy (for good or for bad) than they and most commentators believed, and that the value of their advice was largely to provide rationales for policies adopted on other grounds, which was just as well because the knowledge in back of the advice was sharply limited. ("Such bad food, and small portions as well," as Woody Allen would put it.) He also contrasts positivistic social science thinking with more historical approaches, and argues that thinkers of both types took for granted the standard Cold War assumptions and never questioned the ends to which American power was being applied. But these arguments run through the text in a somewhat scattered fashion rather than being made cleanly and brought into close contact with possible competing arguments.

Because the book deals with ideas and individuals who were in my field, but a generation ahead of me, I hope it is not amiss to start by explaining my relations with them and to concentrate on issue of nuclear weapons and strategic studies. Indeed, Kuklick's story made me feel a bit like Zelig in another Woody Allen movie because with the exception of George Kennan I kept bumping into these people. Although I didn't study with Tom Schelling, he was my mentor and one of the most important intellectual influences in my life. I also was a teaching assistant in Henry Kissinger's graduate course on defense policy and was at the session in which Robert McNamara spoke shortly after his unpleasant confrontation with protesting students, an episode Kuklick describes. I also spent time with McNamara at several academic conferences. I had many conversations with Bill Kaufmann at MIT who Kuklick correctly designates as one of the main developers of ideas about deterrence and who went on to be a major advisor and speechwriter for McNamara. (Like McNamara, Kaufmann was a fierce critic of what he saw as the excesses of the military, especially the Air Force, although it could be argued that some of his ideas about fighting limited wars provided grounds for their demands for ever more weapons.) Dick Neustadt, the only person studied by Kuklick who was concerned with the process of decision-making rather than with the substance of defense issues, was a colleague of mine, and I am very glad that I urged him to publish and reflect on his post-mortem on the Skybolt conflict, a study that Kuklick draws on. At Harvard I also met William Bundy and

Daniel Ellsberg, who were spending the year in Cambridge writing about Vietnam. When the Pentagon Papers appeared in the newspapers, it did not take long to guess that Dan was the source. When I moved to UCLA I had Bernard Brodie as a colleague, and without this experience I probably would not have read his work with as much care as it deserves. At UCLA I also renewed my acquaintance with Albert Wohlstetter, whose seminar I had taken as graduate student in Berkeley. Wohlstetter was a very influential figure who established a key concept I will discuss later but many of whose ideas seemed to me quite wrong-headed and whose legacy lives on in the foreign policy of neoconservatives, many of whom were his students at the University of Chicago. When I moved to New York, I became friends with McGeorge Bundy because of our shared interest in nuclear weapons and commented on drafts of his *Danger and Survival*, which may explain why I think more highly of the book and the man than Kuklick does. It is also worth noting that these men were readily approachable by a young scholar, all were generous with their time, and all listened with some care to my ideas, however foolish. I do not think this blinds me to their shortcomings but I do think they compare rather well to many other academics, intellectuals, and advisors.

Although Kuklick does not go into great detail about each of these people or how they thought, what he does say seems to me generally accurate. Schelling's crisp logic, Neustadt's exuberance and love of politics, Wohlstetter's condescension, and the great confidence coupled with shifting substantive views that characterized McNamara and Ellsberg are caught quite well. But some things are missed and some ideas are misstated. Brodie's odd combination of shyness and assertiveness does not come through, his fierce feud with Wohlstetter is not mentioned although it is of more than personal interest because despite being fueled by differences in personality it had an important intellectual core that Kuklick does not discuss. Wohlstetter's personal magnetism, which played a considerable role in his influence, is also missed. He similarly fails to bring out some of the important ideas and arguments about nuclear strategy. Counterbalancing some of these deficiencies, while Kuklick is critical of those he studies, he never is snide and does not fall into the trap of implying moral or intellectual superiority, as so many academics do.

STRATEGIC STUDIES

Kuklick correctly notes that many of the relevant ideas were developed at the RAND Corporation, or by scholars like Schelling and Kaufmann who spent a good deal of time there. But in fact the fundamental lines of competing arguments were developed previously, in the immediate wake of Hiroshima[1]. In his essay for *The Absolute Weapon*, Brodie argued that nuclear weapons were different from non-nuclear or conventional ones not in degree but in kind. To make a long story short, deterrence would replace defense, and many of the traditional relationships between force and foreign policy would have to change. 1946 also saw the publication of William Borden's *There Will Be No Time*, a book that received less academic attention (although Borden himself went on to exercise great influence as the leading staff member of the Joint Congressional Committee on Atomic Energy). Despite referring to nuclear weapons as revolutionary, Borden's analysis was premised on the claim that they were not, and

[1] For elaboration, see Robert Jervis, *The Meaning of the Nuclear Revolution* (Ithaca: Cornell University Press, 1989, chapters 1-3.

that while many precepts of strategy had changed, the value of military superiority and the danger of vulnerability had not.

These were to be two of themes that Albert Wohlstetter developed so insistently. Brodie's view led to the argument that, once both sides had secure retaliatory forces, military superiority was meaningless (which in the 1960s would become known as Mutual Assured Destruction or MAD); Borden's view produced continual fear that the Soviets might believe they could "fight and win a nuclear war", to use the title of the important article by Richard Pipes, a leading historian of Russia and the Soviet Union who headed the Team B that challenged prevailing CIA estimates in 1976 as being too complacent[2]. In this view, the balance of terror was delicate, to take from the title of Wohlstetter's January 1959 article in *Foreign Affairs* that had great impact on the defense policy elites. The latter view also implies that gaining sufficient credibility for deterrence is difficult, which in turn would predisposed one to believe that meeting perceived Soviet challenges in the periphery was vital, whereas Brodie and his followers thought that establishing the credibility of deterrent threats was much easier. It therefore is no accident, to borrow a phrase from a different intellectual tradition, that many in the latter camp thought that the US did not to fight in Vietnam, while I believe all of the former disagreed.

This story is not central to Kuklick's account, and obviously any author is free to craft the boundaries of his or her own book. But for one that focuses on RAND and defense intellectuals to say little about this fundamental issue strikes me as odd[3]. Furthermore, it supports one of Kuklick's main arguments. To put it a bit dogmatically, while Brodie won the argument within the academic community, the Borden-Wohlstetter school of thought prevailed in the configurations in American strategic forces. One might not know this from reading American public statements (often called "declaratory policy"), but the fear of Soviet military superiority and the desire for multiple nuclear options drove much defense policy after 1960. There was then a major disconnect between what the academics believed and what the Air Force was told to plan for. Although many scholars and analysts at RAND did favor limited options in the late 1950s and early 1960s (as Kuklick notes) and Wohlstetter never lost his enthusiasm for them, the influence of most of the people Kuklick discusses was sharply limited when they came to a different conclusion. But the story does not stop here. Although presidents and their top advisors always felt that the Air Force was slow to develop limited options, they did believe that they would have a significant range of military tools at their disposal if need be. They were wrong: continuing the views of Eisenhower that Kuklick outlines and moved by a combination of bureaucratic concerns and practicalities, the Air Force never thought that limited nuclear war was possible, and it was not prepared to fight in this way. So in a sense Brodie turned out to be right, although not because his arguments persuaded either civilian or military leaders.

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[2] Richard Pipes, "Why the Soviet Union Thinks it Could Fight and Win a Nuclear War," *Commentary*, vol. 64, July 1977, pp. 21-34; for a critical analysis of the Team B episode, see Anne Cahn, *Killing Detente: The Right Attacks the CIA* (University Park, PA: Pennsylvania State University Press, 1998).

[3] My perspective may differ from Kuklick's in being closer to what he calls "practitioners' history" (p. 231) and perhaps I am criticizing the author for not writing a different book, but I think his account fails to give the reader an understanding of many of the ideas and arguments he treats.

The story of the conflict between the two schools of thought bears on Kuklick's main themes in several other ways. First, at a number of points he touches on what can be called the description-prescription tension in international relations (IR) scholarship (pp. 78-88, 138, 190). Many IR theories claim that states both should and do behave in certain ways. As long as behavior and prescriptions track together, this is not a problem. But when they diverge, they not only harm the country (assuming the theorist is correct), but embarrass the theory. As Kuklick correctly says, "[Hans] Morgenthau spent his career blaming American policy-makers for doing what they should not have been able to do--violating the iron laws of politics" p. 78. The point here is that the Brodie-Bundy-MAD theory says that nuclear superiority does not bring foreign policy advantages, yet policy-makers pursued it. The MAD arguments may have been right as prescriptions—and I vigorously seconded them—but they are hard put to explain why the leaders thought differently. Thus while Bundy could argue for the imperative for reaching arms control agreements in order "to cap the volcano"[4] if he was right that what he called "existential deterrence" (derived from MAD) held, then there was no volcano to cap, the Soviets were just wasting their money building a larger force, and there was no good reason (aside from ever-present domestic politics) for the US to follow their example even if an agreement could not be reached. Furthermore, if decision-makers really did believe that nuclear superiority mattered, then presumably their behavior in a crisis would be influenced by it, and so superiority would actually matter.

The description/prescription problem comes up in another way that Kuklick touches on (p. 120) and that is arguably central to the role of theories and ideas in policy-making. This is the possibility that theories and arguments will become self-fulfilling prophecies. One limitation on the analysts' attempts to construct generalizations is that what happens in one case may change what people and states will do later. So if analysts teach leaders about the "lessons" of the past, behavior may come into line with the theories, although whether this will be in the interests of the leader, the country, or the world, is another matter. To take the obvious example, it is at least possible that much of deterrence theory accurately describes American behavior during the Cuban Missile Crisis not because of any specific advice given by the people Kuklick studied, but because government leaders had by then absorbed basic deterrence thinking.

A more ironic example may be the roots of Nixon's "madman theory." He told his advisors that he hoped to put pressure on North Vietnam and the Soviet Union by leading them to believe that he could not be counted on to behave carefully, and this led to his putting the Strategic Air Command on alert in the fall of 1969. Nixon may have come up with this idea on his own, but this tactic was in the air after Schelling pointed out that the "rationality of irrationality" followed from an abstract understanding of bargaining. Furthermore, it was one of Schelling's students, Daniel Ellsberg, who did most to explore this idea by arguing that Adolf Hitler had practiced it (and Ellsberg lectured on the subject in Kissinger's classes (p. 175)). The theory may then turn out to be right, but only because decision-makers have come to believe it. Of course in other cases theories can be self-denying prophesies; for example, if decision-makers believe a certain theory of the causes of war and want to stay at peace, wars that fit the theories may no longer occur.[5]

[4] McGeorge Bundy, "To Cap the Volcano," *Foreign Affairs* vol. 48, no. 1 (October 1969).

A second aspect of the vulnerability issue mentioned earlier opens the way to questioning one of Kuklick's main arguments. Kuklick discusses Wohlstetter's RAND study that explained how American strategic forces in the mid-1950s were vulnerable to a Soviet surprise attack (pp. 60-69), but he misses the broadest intellectual contribution of this study. Although the Air Force asked RAND to answer a fairly narrow and technical (if important) question about the efficiency of alternative schemes for basing bombers and the tankers they needed for aerial refueling, in the course of their analysis Wohlstetter and his colleagues realized that previous thinking was confused in not differentiating between first-strike and second-strike capabilities. A country had the former if it could wipe out the adversary's nuclear forces in a surprise attack; the latter is the ability of a state to absorb a surprise attack and still retaliate with sufficient force to destroy the attacker's society. As Wohlstetter and his colleagues came to understand, a force could be large enough to do a great deal of damage if it struck first, but simultaneously be itself vulnerable to a first strike. Although today the distinction between first- and second-strike capabilities is axiomatic, it was not well understood before Wohlstetter's work. Thus when one reads documents and NSC minutes of the 1950s, they seem strange, if not disorienting, and often obtuse. They talk about the dangers posed by increasing Soviet military forces without making clear whether they are worried about the Soviets gaining first-strike capability against the US, or the US losing its first-strike capability against the Soviet Union. This is not a mere quibble. The way one would deal with one of these problems is very different from the way one would deal with the other. The greater strategic sophistication that we gained from Wohlstetter's distinction (or, to put it less judgmentally, the different framework that we now use) makes it much harder to understand the arguments of the 1950s. We need to see the world through the conceptual lenses that the contemporary policy makers wore, not the ones that have become second nature to later strategists.

This analysis points to a very important kind of influence that Wohlstetter and his colleagues exercised that Kuklick misses because he focuses on specific incidents during the Cold War. If I am right, the Wohlstetter study and the work it spawned gave not only the academic community but also policy makers a new vocabulary and view of the world. These were so intellectually alluring that they became taken for granted.

Furthermore, this way of looking at matters had very important policy consequences. It led to Schelling's argument for the importance and danger of "the reciprocal fear of surprise attack".[6] That is, if both sides had first-strike capability, war could start even if no one wanted it because each side would believe that whoever got in the first blow would win the war. This led to what became the mainstream ideas about arms control, which centered on the importance not so much of the absolute numbers of weapons on each side, but on their being in a configuration that minimized the incentives of striking first. Arms control agreements with the Soviet Union, and especially the sharp limitations on ballistic missile defense, followed. Kuklick might of course reply that this is not cause-and-effect: the fear of surprise attack came from Pearl Harbor,

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[5] James Fearon, "Rationalist Explanations for War," *International Organization*, vol. 49, Summer 1995, pp. 379-414; Erik Gartzke, "War is in the Error Term," *International Organization*, vol. 53, Summer 1999, pp. 567-87.

[6] Thomas Schelling, *Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960), chapter 9.

not the concepts of Wohlstetter and Schelling. Furthermore, the search for stability through both unilateral measures and arms control stemmed less from this theory than from the imperatives to reduce defense spending, lower tensions with the Soviet Union, and show the American public that the Cold War was being managed prudently. There is quite a bit to this argument, and indeed by the 1980s Schelling would correctly lament that arms control was pursing the false goddess of absolute numbers rather than remaining focused on stability.[7]

But I do think that the ideas were important here, especially in the 1960s and 1970s. The fear of mutual first-strike capability gave great impetus and a sharp focus to American policy. It also complicated the Soviet-American dialogue, since the Soviets did not adopt the American ideas and saw the world quite differently, preferring a theory of war that focused less on weapons and more on politics. This caused American negotiators great frustration, and they saw much of their task as "educating" their Soviet counter-parts to understanding the "realities" of nuclear strategy. The frustrations also came out in the famous dialogue between McNamara and Soviet Prime Minister Kosygin at the Glassboro Summit in 1967.

But stability would create problems of its own, and this is another area in which the academics may have had influence that Kuklick unfortunately glosses over and, in a minor way, confuses. In the "stability-instability paradox,"[8] although mutual second-strike capability would make all-out war much less likely, this very stability would permit a country with conventional superiority to engage in provocations at this lesser level of violence. In other words, taking advantage of the shield of nuclear parity, the Soviets could invade West Europe. Wohlstetter and his colleagues argued that the only answer was for the US to develop the ability to fight and win a limited nuclear war. In Herman Kahn's terminology, the US needed superiority at all possible rungs of the escalation ladder.[9] The implicit assumption here was that, contrary to the belief that underpinned the Eisenhower policy, a limited nuclear war could be kept under control if it were fought carefully enough. Escalation was like a ladder rather than a slippery slope.

At first, it seemed as though this conclusion followed ineluctably from the idea of the stability-instability paradox, a conception that almost all academics and policy-makers accepted. But it did not, as Schelling showed in one of his most important contributions to IR thinking and, quite possibly, to policy. He showed, logically if not empirically, that actions did not have to be militarily effective in order to help prevail in a crisis. Rather, such confrontations were "competition in risk taking" because they were inherently dangerous, and this meant that states could employ what he called "threats that leave something to chance"--i.e., states could take actions like moving troops or using limited force that would increase the chance of inadvertent

[7] Thomas Schelling, "What Went Wrong with Arms Control," *Foreign Affairs*, vol. 64, Winter 1985/1986, pp. 219-34.

[8] Glenn Snyder, "The Balance of Power and the Balance of Terror," in Paul Seabury, ed. *The Balance of Power* (San Francisco: Chandler 1965), pp. 184-201.

[9] Herman Kahn, *On Escalation: Metaphors and Scenarios* (Baltimore, MD: Penguin, 1968).

and undesired escalation.[10] Indeed, states employ this tactic even if they do not want to because any military action in a crisis creates some risk that things would get out of control.

The point here is that the US could protect its West European allies despite nuclear parity and Soviet conventional superiority if it convinced the Soviet Union that it would respond to extreme pressure or an invasion by taking actions that might lead to all-out war. Limited options might be needed, but unlike those required under Wohlstetter's approach, military sufficiency was not required. We can of course debate whether Schelling's argument is correct either descriptively or prescriptively. It remains unclear how often decision-makers did act in this way and we can obviously debate the conditions under which such a policy would be wise. But there is at least a prima face case here: as I noted earlier, Nixon claimed to be acting according to his madman theory, and a memo from Schelling outlining his ideas went into Kennedy's weekend reading packet in July 1961[11] and US policy in the Berlin crisis changed accordingly shortly thereafter. Although many additional forces were at work, Bundy noted that the paper made a "deep impression" on the president and the ideas may have given Kennedy more confidence that he could indeed face Khrushchev down. It is a hypothesis worth exploring that American—and perhaps Soviet—leaders saw risk, escalation, and safety differently after Schelling's ideas gained currency.

Finally, it is worth noting a possible second-order relationship between theory and policy. Leaders might react differently to risky actions taken by the other side if they believe that the other is consciously exercising the threat that leaves something to chance as contrasted to cases in which they think the adversary is acting naively. Exactly how the inferences and responses would differ is a digression here, but the point is simply that how an actor interprets what the other is doing depends not only on the implicit theories that the perceiver holds but also on those he imputes to the actor. The last point highlights what I think was the central contribution of many of the analysts Kuklick discusses, one that he barely mentions. This is the role of game theory, not in its particular mathematical details (one of which Kuklick gets wrong on p. 25), but in the essential point that international politics and much social life is composed of interactions in which each person or unit bases its behavior in part on its anticipation of how others will behave, knowing that others are doing likewise. Of course some of this was understood by scholars and leaders long before the theory was put in mathematical form; the diplomatic documents of the late 19th and earlier 20th century could be the basis for a game theory textbook. But not only has game theory revolutionized economics, political science, and much of biology, but I think it is quite likely that most national leaders think about their interactions differently than they did before, paying more attention to their expectations of how others will behave and to how others are anticipating their behavior. But partly because Kuklick does not

[10] Schelling, *Strategy of Conflict*, pp. 187-208); Schelling, *Arms and Influence* (New Haven, CT: Yale University Press, 1966), pp. 92-125, also see Jervis, *Meaning of the Nuclear Revolution*, pp. 80-95; Kuklick notes that some theorists talked about the manipulation of risk, but his treatment is too brief and he associates the concept with Kaufmann (pp. 107-09.)

[11] Thomas Schelling, "Nuclear Strategy in the Berlin Crisis," *Foreign Relations of the United States (FRUS), 1961-1963*, vol. XIV, *Berlin Crisis, 1961-1962* (Washington, DC: Government Printing Office, 1993), pp. 170-72; an academic version appeared as "Nuclear Strategy in Europe," *World Politics*, vol. 14, April 1962, pp. 421-32.

seem to appreciate the basic points of game theory, he does not explore the argument that the incorporation of this way of thinking overshadows many of the more specific ideas he considers. I am not sure that such a case could be sustained, but it should not be ignored. (Again, the question of whether game theory had a large impact is separate from whether its influence was benign or malign.)

VIETNAM AND KISSINGER

Kuklick devotes parts of several chapters to the linked topics of Vietnam and Henry Kissinger and here I am in general agreement with his argument that the security studies theories he discusses played at most a minor role. Critics from the left argue that these theories led decision-makers to accept the domino theory, see North Vietnam as a puppet, and believe that preventing a Communist victory was vital; critics on the conservative end of the spectrum fault the theories for misleading policy makers into believe that subtle bargaining tactics and restraints from American bombing are responsible for the US defeat. Although Kuklick does not discuss these arguments head-on, he implies both are incorrect, and I agree with him. While I would argue that the domino theory was indeed important and is consistent with some aspects with the theories Kuklick discusses, the basic point predated the analysts' intellectual investigations. The Munich analogy was central, it was more a cause than an effect of deterrence theories, and it took hold early in the Cold War.

Deterrence theorists were divided on the war, and one cannot even argue that those who were most involved with the more technical aspects of security studies supported it and the more historically inclined intellectuals did not because Schelling opposed the war and Kissinger favored it. The argument that the way the war was fought reflected RAND-style theorizing also is exaggerated. Restraints there were but they were largely imposed by the (exaggerated) fear of Chinese intervention and opposition from allies and domestic opinion. In broad outline, the way the US fought and bargained was not so different from its behavior in Korea, years before the creation of much of the intellectual apparatus that is often blamed for Vietnam.

On one crucial point, American leaders went wrong because they disregarded a fundamental teaching of bargaining theory, although unfortunately few of the theorists picked this up. As Kuklick notes, but unfortunately does not stress, many members of the American elite believed that the US would prevail in Vietnam because it was so much stronger than the adversary (p. 43). But the bargaining theories developed in the preceding years had shown that strength normally conceived was not the only factor at work. Just as important as the ability to punish the other side was the willingness to bear costs oneself. Put another way, strength of motivation was crucial, and here North Vietnam had the upper hand because prevailing in the South was much more important to it than it was to the US. This is the fundamental truth behind the important exchange with which Harry Summers begins his book on Vietnam: as the war was ending, an American colonel said to his North Vietnamese counter-part that "You never defeated us in any battle," and was told in response, "That may be so, but it also is irrelevant."[12] The North Vietnamese leaders were willing to have their country absorb enormous punishment rather than concede. The point of the threat and use of force (and the application of other instruments

[12] Harry Summers, *On Strategy: A Critical Analysis of the Vietnam War*, (Novato, CA: Presidio Press, 1982), p. 1.

of influence as well) is to bend the other side to your will. The bargaining theory being developed indicated quite clearly that the choice of whether to comply lay with the other side, however. Again, this point is an old one, but it was made more explicit in many bargaining theories, and if they had been influential decision-makers might have chosen differently.

But I agree with Kuklick that they probably would not have. Although we still debate the forces moving the American leaders, they were surely deeper and more powerful than the ideas developed at RAND and the academies.

Kuklick's discussion of Kissinger is interesting, and he rightly devotes several pages to his B.A. thesis "The Meaning of History" (pp. 184-88), which he sees as a work dealing as much with philosophy as with history and political science. He also is correct to note that Kissinger's later work was derivative and inconsistent. While agreeing with Kuklick that Kissinger brought a powerful set of ideas with him to Washington and generally acted according to them (pp. 226-27), I would raise some questions. Not only were the broad conceptions of the policy attributable more to Nixon than to Kissinger, it is hard to link the latter's geostrategic outlook to what he had written before taking office. The influence of his earlier ideas was perhaps greatest in how he operated--his contempt for the bureaucracy is the one consistent thread in his scholarship, behavior in office, and memoirs. But it is harder to trace the substance of important policies, such as linkage, the pursuit of détente with the Soviet Union, or mediation in the wake of 1973 Middle East war to his previous theories. Indeed, hard-line critics at the time and, even more after the end of the Cold War, criticized Kissinger for his lack of attention to the domestic sources of foreign policy in both the US and the USSR, and this should have been salient to him from his doctoral dissertation and first book on the Congress of Vienna.

THE ROLE OF IDEAS AND WAYS OF THINKING

In closing, let me return to two of Kuklick's themes: the limited impact of RAND-style theorizing and the limits on its acuity. Kuklick stresses that in most cases the main role of the ideas was not shaping how decision-makers thought, but giving them handy rationalizations for their behavior. After events occurred many of the theories were also useful for either applauding decision-makers if things went well or shifting blame if they went badly. "Like other people, diplomats needed some moral and political discourse to validate their efforts, to have policies make sense to themselves and others. Policymakers did what they wanted, though they had to come up with reasons for what they did" (p. 225). I think there is much to this, and it is a useful corrective to the common accounts that either credit or blame RAND and academics for the course of the Cold War. But as my earlier discussion indicates, the influence may be greater than Kuklick portrays.

Kuklick correctly argues that it often appears as though leaders are following advice when actually they have adopted the formulations propounded by intellectuals as rationalization and justifications for what they had decided to do on other grounds. This is often correct, and Kuklick could have gone further and noted that advisors are often selected for this purpose. For example, in the run-up to the overthrow of Saddam, the White House frequently consulted with Middle East experts Bernard Lewis and Fouad Ajami, who argued that Saddam had to be stopped, that Iraq could then be readily reconstructed, and that this would produce good effect

throughout the Middle East. But it is obvious that these people were called on because they would tell the administration what it wanted to hear rather than their persuading it. On at least a few occasions this is not the case, however. Sometimes advisors can produce surprising opinions, as when the "wise men" told President Johnson that his policy in Vietnam was failing after the North Vietnamese Tet offensive (in retrospect, a questionable judgment). More interestingly from Kuklick's perspective, on at least one important occasion advisors were selected for one purpose but ended up having much broader influence. As Kuklick notes, McNamara brought a number of people from RAND into the government and he subsequently, if briefly, adopted their ideas about the need for flexible response and multiple nuclear options. This does not appear to be a case parallel to that of the White House calling in Lewis and Ajami. McNamara drew on RAND because of its arguments for cost-benefit analysis as a way of making weapons procurement decisions and managing the Defense Department. It was essentially an accident that these people also brought with them substantive views on nuclear strategy that McNamara found appealing.

As the title of his book indicates, Kuklick is not sorry to see that the intellectuals' influence was limited. Their theories assumed excessive rationality, flattened differences between people and between countries, adopted stereotyped views of the Soviet Union, and never questioned the goals and purposes of American policy. "While they professed a deep understanding, they actually groped in the dark" (p. 15). There is more than a bit to this, but the indictment is overdrawn. These analysts were no more guilty of failing to understand the Soviet Union or to explore questions of whether the United States was acting for good or for evil than were most contemporary scholars. They may have had an inflated view of how much they knew (p. 127), but this also does not differentiate them from other scholars.

Throughout, Kuklick alludes to the contrast between RAND-style theorizing and historically-based thought (in addition to the pages listed in the index, see pp. 204-215). But while he clearly prefers the latter (he is, after all, a historian), he is never completely clear on what he means. He also concludes that the anti-positivists that he studied did not come to systematically different conclusions, let alone better ones, than did those who fully embodied the RAND-style (pp. 228-29), and he also says that "it would be difficult to make a case that the conduct of American foreign policy was worse in the postwar period than that of the Europeans who in general had rejected positivist social science, or the Russians who adhered to a Marxist social science" (p. 229). Thus while Kuklick argues that "the historian must wonder about the point of the exercise of strategic studies" (p. 228), one may wonder whether his own distinction takes us very far. Relatedly, Kuklick's summary of the possible role of knowledge in aiding decision-makers and countries is justifiably ambivalent. He notes "the role of hubris among the intellectuals" (p. 229), and concludes that "the best traits to be inculcated into specialists are humility and prudence, just the traits that vanish with their education and growth in expertise" (p. 230). I would not disagree and indeed would just add that (positivist) empirical research has revealed links between cognitive style and a willingness to adjust beliefs to new and discrepant information.[13] It might be best for policy and scholarship if both groups gave up the hope for expert advice. But this would silence critics as well as advisors, leave open the question of what

[13] Philip Tetlock, *Expert Political Judgment: How Good is It? How Can we Know?* (Princeton, Princeton University Press, 2005).

ideas decision-makers should draw on, and render a more pessimistic judgment about the possibilities of systematic knowledge than most of us are likely to be willing to accept.

In recent years, IR scholars and diplomatic historians have had several interesting and I think productive dialogues.[14] There is much more to be said on this subject, and I hope Kuklick explores it in another book as stimulating as this one.

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[14] See, for example, Colin Elman and Meriam Fendius Elman, eds., *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations*, (Cambridge, MA: MIT Press, 2001); readers will not be surprised to learn that I thought especially well of the concluding exchange by Paul Schroeder and myself.